

A BODY MUST HAVE FLESH AND BONES

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I agree with the gist of Staddon's message, as I agree with Branch's (1992). But, just as "one finds God in the details," there also lurks the devil. Exhortations concerning research methods are about as effective in changing behavior as Sunday sermons. But we finish both feeling improved; and although a good model, either from the laboratory or from life, is more efficacious, talk is cheaper. Here's my two bits.

Environment-Based Theorizing

In the experimental analysis of behavior, the term *history* usually refers to *history of reinforcement*; it is not a blurring of Darwin's distinction between immediate and ancestral adaptation, but a principled definition of the domain of our discipline—the variance found in the behavior of individual subjects (not between individuals or between species), in particular the variance in such behavior (whether we call it operant, respondent, or adjunctive) attributable to contingencies of stimulation. We must draw limits somewhere in our field. As our knowledge develops, we are better able to decide which partitions of the field minimize the residual variance in our descriptions of its phenomena. Periodically the limits should be redrawn, and pressure to do so now provokes the present controversy.

Whereas Darwin sometimes invoked "organism-based" hypotheses, he preferred environmental ones when they were available (found throughout his marvelous works on emotions, earthworms and climbing plants). It is a similar bias that Branch imposes, for similarly good reasons. But couching the issue as one of environment versus organism is obtuse, as the essence of behavior is interaction of organism with environment; environments and organisms are neither exclusive nor even orthogonal. A better distinction is between *in-*

dependent variables that can be measured and controlled and *state variables*, simple hypothetical constructs usually consisting of a single number, which must be inferred. This is Staddon's position, but I think also largely what Branch meant by his distinction. All protagonists would agree that, when available and accurate, an account consisting of only independent variables is preferable. They could probably be brought to a negotiated agreement on use of a state variable when the power it adds is worth the decrease in parsimony it entails.

Construction license. Staddon's criterion for invoking hypothetical constructs is apparently satisfied when a "a puzzling phenomenon . . . is thereby made more comprehensible" (p. 440). But if that were the whole story we'd all be psychoanalysts, not behavior analysts. Every "A-haa!" should be chased with an "Oh-oh, now just what do I know?"

Discovering inventions. Although it is true that "not all theories can be arrived at simply from orderly arrangements of data" (p. 440), all scientific theories *are* arrived at by arranging and rearranging data. Those rearrangements may occur either while we are physically manipulating representations of the data or while we are sleeping on them. Because the unconscious processes are so ill-defined, we should not be too rigid in insisting they are different in nature from perceptual processes (e.g., by insisting they are "inventions" rather than "discoveries").

Consider Bethe's (1989) discovery of the process by which the sun produces energy:

I found the carbon cycle in a very systematic way. . . . I had to look for a reaction which involved atoms with higher potential barriers [than hydrogen]. So I went systematically through the periodic table but . . . whatever atom I used . . . would be destroyed in the process. . . . Finally I got to carbon, and as you all know, in the case of carbon the reaction works out beautifully. One goes through six reactions, and at the end one comes back to carbon. (pp. 11-12)

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Bethe simplified the problem he was given, started from what was known, had well-defined criteria for a solution, and then searched systematically. "That's how I invented the carbon cycle," Bethe says, "or should I say 'discovered' " (Horgan, 1992, p. 40).

Consider next Bethe's (1989) account of the invention of renormalization, a method of fundamental importance to quantum electrodynamics, and which, when generalized by Feynman and others, produced the most accurate theory in physics:

I thought that it ought to be possible to get Lamb's result by applying the idea of Kramer's. So on the train . . . I wrote down some elementary equations of radiation interaction and found out that the effect . . . would involve the logarithm of the energy. Inside the logarithm, the numerator was some energy which I did not know, while in the denominator, there was something like the binding energy of the electron in hydrogen. . . . This sounded very hopeful. (p. 13)

Discovery or invention? Until we have a viable model of the creative process, we should not rule out the possibility that it results from a "confluence of forces at a suitable locus," like turbulence after the join of rivers or insights after the flow of data.

Newtonian Mechanics

"The problem with Newtonian mechanics as a model for behavior theory is that it is entirely *ahistorical*," whereas "response rates . . . at one time denote a different *system state* than an identical set at another time" (p. 440). But Staddon can't *know* that (unless he assigns every possible history a different state, which bankrupts the notion); and even if it were true, it is usually irrelevant to those whose interest is in understanding behavior. He makes this clear himself in his later cogent description of "equivalent histories." If one is interested in predicting what an animal will choose tomorrow, the best (most accurate and parsimonious) predictor *may* be simply its choice today; or we *may* need to include specification of an internal state (possibly modified in the interim). The best predictions are those that depend on the fewest hypothetical states.

All systems above the level of the quantum are historical. The precision of Newton's predictions of the behavior of the heavenly bodies

is undermined by the tides they draw in their neighbors, which inevitably affect their periods. This is a dissipative effect whose magnitude is not predictable from conditions that are knowable *a priori*, but a coupling constant may be assigned as a historical state variable to generalize the predictions. In the worst case (the planet Pluto), such interactions give the orbit chaotic components. No serious critic has held such imprecisions against Newtonian mechanics; these are limits, not repudiations. In like manner, the cosmological term (λ) in general relativity depends on the quantity of matter in the universe, another "historical state variable" (i.e., contingent fact). Thus, intrinsic historicity is not the issue (because it is ubiquitous): Finding an account that covers the simple case, that at first sidesteps the more path-dependent aspects and that can be generalized progressively to embrace them, is the winning game plan.

Sometimes the causes of phenomenon are so obscure that we call it a Markov process. But because we cannot explain the variance in terms of causal paths does not mean the processes are not historically determined; on the contrary, we have buried the paths by invoking probabilities. Probability is a euphemism for all that we don't know about a phenomenon.

Imagine some far-sighted scientist 300 years ago saying, "unless we are content to remain forever at the level of static principles, and thus abandon any hope of understanding the process of change, something beyond the Newtonian model must be found," and then starting to work on general relativity! He would not succeed, or even be understood. We must focus on what is in front of us. Newton's approach provided a brilliant model, both of mechanics and of the scientific process, for 250 years. Have we yet achieved a model of behavior that provides both static and dynamic principles that are as good as Newton's? I think not. But replace the word "change" with "learning" and read the above impatience on page 441 of Staddon's article.

If we wish to study the play of colliding objects, we will get further with billiards than with tomatoes, because deformations of the internal state of the latter render such collisions inelastic. The trick is in finding "good preparations" that forestall the need for ad hoc characterization of internal states—something

Skinner himself emphasized. We must first seek reliability and simplicity in our phenomena and their descriptions; once that is found, we may look for added specifications involving state variables that make our account more general. If we seek generality before simplicity, we shall succeed only in generalizing our confusion.

Language

Well said!! (especially the first two paragraphs). Just as we are trained to limit our numbers to their significant digits (a lesson never learned by Clark Hull!), so too should we limit reported precision to "fit the needs of the moment." Terms usually refer to entities having both location and spread: Not only should we not characterize the wavelength of yellow light to too many decimal places, we should report the range to which we ascribe that hue. Equally important is accuracy: A characterization should be centered over the thing it refers to, as well as qualified as to the precision. Both first and second "moments" of our description should match those of the phenomenon we describe.

But there is a need for stability in definitions once they are achieved; just as a body needs both bones and flesh, science needs both stability and flexibility. Stability is not the same as rigidity. Usage should evolve to respect new understandings, but it should not drift. The discipline provided by standard terms both forces novices to think and permits experts to communicate efficiently. For without stable conventions, how would we understand terms such as *history*, *marginalization*, and *two-armed bandit*?

Poincaré (1905/1952) discussed the creative derivation of conventions that respected the orderliness in nature:

From them, indeed, the sciences derive their rigor; such conventions are the result of the unrestricted activity of the mind. . . . Experience guides us. . . . Our laws are therefore like those of an absolute monarch, who is wise the consults his council of state. . . . Some have set no limits to their generalizations, and at the same time they have forgotten that there is a difference between liberty and the purely arbitrary. (p. xxiii)

Conventions must, by definition, be conservative, and respect the wisdom of the ages; but

they should not be reactionary, to stifle the creativity of the moment.

The CE Model

This is a fine demonstration of the importance of state variables within a parsimonious theory. Staddon has learned the lessons of parsimony so well that he may not see the importance of continual emphases on it. We all know that you can fit any data with enough parameters in a mathematical model, *if* you know what you are doing! We should consider each cognitive, verbal, hypothetical construct to embody at least two parameters (mean and spread); it only takes a few of those to fit any data, and in this case, you don't even have to know what you are doing!

But parsimony—stinginess—is a mean-spirited virtue. Elsewhere I have argued that it should be replaced with prudence (Killeen, 1987). We should feel free to explore hypothetical constructs ad libitum. But we should feel constrained to make each one we keep pay its way in conceptual clarification and predictive power. Hypothetical constructs are like predictors in a stepwise regression equation; one quickly comes to the point at which they do more harm than good, and the prudent will venture no farther.

Conventional Wisdom

Conventions are rules of thumb for conduct that evolved because observing them is more likely to be beneficial than breaching them. Staddon argues that the conventions of the experimental analysis of behavior, epitomized in the editorial policies of this journal, can be improved. That you are reading his contention and commentaries on it here suggests that its editors are listening. What types of improvements? Poincaré (1905/1952) offered some conventional wisdom on that question: "Experiment is the sole source of truth. It alone can teach us something new; it alone can give us certainty. These are two points that cannot be questioned." Then he observed that a catalogue of experimental data is not a science: "Science is built up of facts, as a house is built up of stones; but an accumulation of facts is no more a science than a heap of stones is a house." "A good experiment . . . teaches us more than an isolated fact. It enables us to predict, and to generalize. . . . The circum-

stances under which one has operated will never be repeated. . . . To predict . . . we must invoke the aid of analogy" (pp. 140–142). The name of scientific analogy is *theory*. The role of theory is to suggest what facts should be grouped with what other facts; which histories are likely to be equivalent; how to generalize and what must be invoked—new measurements or hidden variables—to accomplish the successful generalizations that we call "laws." It is time for a closer coordination between experimental and theoretical analyses of behavior.

The meaning of words must change more slowly than the things we say with them. But change they must, as new understandings flesh out the classical skeleton of the experimental analysis of behavior. Staddon's article is a valuable call to realign our conventions to respect the evolving interplay of fact and theory;

to reinterpret our basic constructs in light of what we have learned since 1938; to rejuvenate what has become, like, alas, so many of us, a middle-aged corpus.

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